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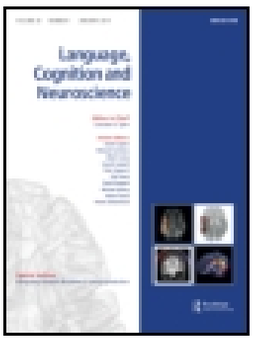

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COMMENTARY

The neuroanatomy of bilingualism: will winds of change lift the fog?

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ABSTRACT

This commentary on Garcia-Pentón, Fernández García, Costello, Duñabetia, and Carreiras [2015. The neuroanatomy of bilingualism: How to turn a hazy view into the full picture. *Language, Cognition and Neuroscience*] suggests that their review may have understated the inconsistencies among studies comparing the neuroanatomy of bilinguals to monolinguals. If their recommendations for better and more consistent methods, larger sample sizes, systematic investigation of various types of bilingualism, and more longitudinal studies were followed the structural picture should become clearer. The main thrust of the commentary is that this clearer picture of the structural changes caused by bilingualism is unlikely to inform the debate over bilingual advantages in executive functioning because: (1) there is no direct mapping between brain function and cognitive function, (2) interpretation of structural differences must rely upon unambiguous alignment with behavioural performance advantages, and (3) the current body of evidence supports the conclusion that either bilingual advantages do not exist or that they are restricted to very specific and undetermined circumstances.

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The title of the review by Garcia-Pentón, Fernández García, Costello, Duñabeitia, and Carreiras (2015) primes readers for a set of recommendations that will turn a "hazy view" of the neuroanatomy of bilingualism into a "full picture". The central empirical conclusion is one that will surprise many readers and induce some to disagree. To paraphrase, Garcia-Pentón et al. conclude that the experimental evidence for structural changes in the brain due to bilingualism is "relatively weak" in that consistent and reproducible structural changes occur in only a few regions such as the inferior frontal gyrus (IFG).

How hazy is this picture?

To this observer it is like a tule fog, the thick ground fog that settles in California's great central valley during the winter. Here is a further distillation of the Garcia-Pentón et al. review. Among nine studies looking for grey matter (GM) differences across the whole brain three reported no differences at all, three reported GM changes in the IFG, and three found changes in each of three different regions. Among eight studies using region-of-interest analyses two showed increased GM in the right IPL, and the remaining six showed idiosyncratic results. Among nine cross-sectional studies of white matter several showed differences in either the Corpus Collosum or Inferior Fronto-Occipital Fassiculus, but with respect to the direction of the differences four showed increases in fractional anisotropy while three showed decreases. Thus, it takes fog lights and consideration of some functional connectivity studies to conjecture that " … if there are bilingualism effects, they may be evident not as a change in the volume of a region, but as the connections between the different regions of a circuit".

How do you lift the fog?

One set of recommendations surrounds the methods for image processing and statistical analysis. I am not qualified to comment on the relative merits of the alternatives, but from the perspective of a former human-factors engineer there is always a tension between standards (or even guidelines) on the one hand and innovation and specialisation on the other. Thus, although Garcia-Pentón et al. are undoubtedly correct in pointing at the differences in methods as one of the causes of inconsistent results, I doubt that greater consistency in methods are on the horizon. Another identified culprit is variation across studies in terms of when L2 is acquired, the level of L2 proficiency, and how it is used. The recommendation is excellent and bilingualism researchers of all stripes should carefully describe the language characteristics of their participants and match the groups on other factors such as

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socioeconomic status, culture, immigrant status, and so forth. Another recommendation is to invest in more longitudinal studies. It will be interesting to see if the plasticity revealed during the first months of immersion or training are the foundation for the reorganisation needed to coordinate two languages at high levels of fluency or if these early changes give way to a markedly different changes on the march to balanced bilingualism. If initial changes are different from those occurring later and not part of a smooth continuum, this would place a limit on the contributions from longitudinal studies.

Lack of power to blow the fog away

It has been a decade since Ioannidis (2005) startling claim that many and possibly most of the conclusions drawn from biomedical research are probably false and that one of the major reasons was the use of flexible statistical analyses and running small studies with low statistical power. A more narrow examination of neuroscience research (Button et al., 2013) showed that the average statistical power is very low and that low power not only reduces the chances of detecting a true effect, but also reduces the likelihood that a statistically significant result reflects a true effect. In this context it is gratifying that Garcia-Pentón et al. call for "higher numbers" of participants, but I would encourage them to be more specific. Based on the studies listed in Tables 1 and 2 the number of participants per language group ranges from 8 to 33 with a mean of 18 and median of 15. Imagine that in addition to the measures of neuroanatomy each of these studies compared monolinguals to bilinguals on a behavioural measure of EF. Based on the meta-analysis of de Bruin, Treccani, and Della Sala (2015) the mean effect size (ES) for bilingual advantages is .3 at most. Using this value of ES, a desired power of .80, a standard alpha of .05, and a two-tailed test; then one needs 176 participants in each group. If instead we generously assume a medium ES of .5 and relax desired power to .67, then the required number of participants decreases to 48 per group. Putting this together I would recommend that simple behavioural tests for bilingual advantages should have at least 48 participants per group and that as many as 176 would in no sense be overkill.

Should the sample sizes be the same when testing for structural differences between bilinguals and monolinguals? Exploring for differences between bilinguals and monolinguals in neuroanatomy complicates the power analysis as a correction for multiple comparisons comes into play. On a different track, there may be a reasoned argument and evidence that the small ESs that appears to characterise the behavioural differences are larger in the neural data. I would encourage the Basque Center on Cognition Brain and Language (BCBL) (and other leading groups) to foster the expectation that future studies will discuss and justify the selected samples sizes and, in addition, to develop numerical guidelines for selecting sample sizes in studies comparing bilinguals to monolinguals with respect to neural function and structure.

Can a clear structural picture be a full picture?

As a cognitive psychologist interested in the relationship between bilingualism and EF I am sceptical that a clearer structural picture will contribute to understanding how acquiring and using a second language produces bilingual advantages in EF. It is perhaps ironic that the BCBL group is far more optimistic given that the reasons for my scepticism are shared reasons that appear to impose clear limits on the valid inferences one can draw from neural results.

At the core is the shared assumption that bilingual advantages are assertions about behavioural performance and that the veracity of those hypotheses must be adjudicated at the behavioural level. Our arguments emphasised that neural measures are often ambiguous (e.g. are increases in fractional anisotropy associated with better performance or worse?) and that interpreting a neural difference as a "bilingual advantage" is only justified when there are behavioural differences that align with the neural differences (Paap, 2014; Paap, Johnson, & Sawi, 2014, 2015; Paap & Liu, 2014; Paap, Sawi, Dalibar, Darrow, & Johnson, 2014). Garcia-Pentón et al. agree: "Supporting evidence for an advantage should involve showing that these differences are accompanied by unambiguous behavioural data substantiating a cognitive gain or, for instance a demonstration of how these differences mitigate normal or pathological cognitive decline in the elderly." Duñabeitia and Carreiras (2015) are even more direct: " … the inconsistency of behavioral findings cannot and will not be settled by structural or functional brain differences". The difficulty, as they simply express it, is that "there is no direct mapping between brain structure and cognitive function" that enables one to link structural changes in a region or network to changes in cognition.

Another critical assumption underlying a causal explanation for why bilingualisms should enhance EF is that language control is subsidiary to general cognitive control. Thus, it makes sense to look for that overlap between these two abilities in bilingual brains and, indeed, the evidence appears solid for a distributed fronto-pariental network (De Baene, Duyck, Brass, & Carreiras, 2015) involved in both types of control. However, Garcia-Pentón et al. caution that: " … demonstrating that

both language and cognitive control mechanisms overlap does not necessarily imply a bilingual advantage. Thus, showing more than neuroanatomical differences between bilinguals and monolinguals is needed to underpin any possible bilingual advantage".

Is the "clear" picture an Escher staircase to understanding the relationship between bilingualism and EF?

There is good evidence that bilingual brains are different from monolingual brains, but those differences are hazy. Imagine, that by following the recommendations of Garcia-Pentón et al, the structural picture of those differences becomes clear and consistent. Just like the men climbing the Escher staircases we may find that we are no closer to our goal (viz., understanding the relationship between bilingualism and EF). This predicament will occur if those different structures do not align with differences in performance on tasks requiring EF. Such a state of affairs would suggest that bilingualism induces changes in structure, but that the changes serve other (non-executive) functions that neither improve nor impair general cognitive control. Why those structural changes develop and the consequences they have for non-executive functioning would, of course, be interesting in their own right.

Might there truly be no bilingual advantages in EF?

We may be well on our way to building the Escher staircase. Here is a brief review of why one should be sceptical that bilingual advantages in EF truly exist.

First, consider the switching component of EF. There is no consistent behavioural evidence for a link between language switching and general task switching. Correlations between very similar instantiations of the two types of tasks are often nonsignificant and the effects of ageing on one ability can dissociate from the other (Calabria, Hernández, Branzi, & Costa, 2013). More directly, Paap et al. (2015) review the case that Prior and MacWhinney's (2010) seminal study reporting bilingual advantages in switching costs is exceedingly difficult to replicate, especially in studies using large sample sizes.

The same bleak picture of the status of bilingual advantages emerges for the inhibitory control component of EF. As described by Garcia-Pentón et al., the researchers at the BCBL have been at the forefront of doing excellent large-scale behavioural studies that have yielded null results and seriously challenge the bilingual advantage hypothesis (Antón et al., 2014; Duñabeitia et al., 2014). Consistent with these studies and many others both Paap et al. (2015) and Hilchey, Saint-Aubin, and Klein (in press) conclude that there is no compelling evidence for a bilingual advantage in inhibition or conflict monitoring.

As Duñabeitia and Carreiras (2015) put it "as far as the universality of the so-called bilingual advantage is concerned, *acta est fabula*". However, they hold open the possibility that consistent advantages may emerge for specific types of bilinguals, namely, those who acquire L2 late in life. Paap et al. (2015) review the efforts to date to test for the specific effects of L2 proficiency, age-of-acquisition, and multilingualism and conclude that if bilingual advantages do exist they are currently restricted to undetermined circumstances. Garcia-Pentón et al. suggest that an improved research agenda (following their excellent recommendations) may facilitate finding those undetermined circumstances. I applaud their optimism, but do not share it. Vive la différence.

Disclosure statement

No potential conflict of interest was reported by the author.

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